Section of Epidemiology and State Medicine.

President—Dr. G. S. Buchanan.

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PRESIDENT'S ADDRESS.

Epidemics of the Eastern Campaigns.

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When in 1915 the Section appointed me its President I was away in the Mediterranean, and as events proved it was impossible for me to do my duty to the Section during any part of last session. This is consequently the first occasion I have had to thank the Section for its nomination, the honour of which I am deeply sensible; the more so as, in the old Epidemiological Society, whose work we continue here, my father was an original member, and President from 1880 to 1882, and I have myself found so much help and inspiration from the meetings and discussions of the Society and Section ever since my membership of them.

The principal work of the Section has always lain in the study and discussion of the broad facts of the incidence and natural history of epidemic diseases, and I have thought it might be useful in this address to give a brief review of those which, within my experience, have been most in evidence in the Eastern campaigns. My opportunities for knowing them have been obtained as a member of a Committee, described under various names, but usually as the Medical Advisory Committee, which was dispatched by the War Office in May, 1915, under the chairmanship of Colonel W. Hunter, for service with Surgeon-

General Babtie in the Mediterranean area—meaning, at that time, Egypt and Gallipoli—with a wide reference for consultation on the prevention of epidemic diseases.

After preliminary work in Egypt this Committee went to Mudros, and to each of the several occupied areas of the Gallipoli Peninsula. In November, 1915, the Committee was sent to Salonica, where British forces had lately been landed, and visited the corner of the Balkans which lies above Lake Doiran. A more comprehensive visit to the forces in Macedonia was made in the following May (1916) the interval being occupied by work in Egypt. Later in the year the bulk of the Committee, with Lieutenant-Colonel Andrew Balfour as Chairman, was dispatched to Mesopotamia, which we reached at the end of August. Here our inquiries took us up the Tigris as far as the outskirts of Kut-el-Amara, which at that time was the furthest point of the British force; up the Euphrates to a corresponding point; up the Karun River, and to various base areas. The period was that of the re-organization and strengthening of the Mesopotamia forces which preceded the great and successful attack on the Tigris front at the end of February in this year, and the subsequent advance to Bagdad. A return via India and Egypt completed the work of the Committee which returned home last May after two years' duty.

I feel that in some ways it might be more profitable to the Section if from all these wanderings I had picked out a single disease or a single epidemic for observations. But any really intensive local study of this kind was hardly practicable under the conditions of the work, and my available notes on any one outbreak have too many lacunæ to be satisfactory. And I believe that on the other hand a review of some general impressions of all the main epidemics met with will not be without interest; there is value sometimes even in a *catalogue raisonné*. I will arrange it by first referring to epidemics of excretal origin, dysentery, cholera, and so on; next, malaria and trench fever; then epidemics which, like sandfly fever, are particularly Eastern; with a note, finally, on the prevalence of diseases due to food deficiency.

Dysentery and Diarrhœa.

I may begin with dysentery, and dwell on it somewhat longer than other diseases, on account of its ubiquity and certain special features which it presents. The great dysentery outbreak of the Eastern campaigns was, of course, that at Gallipoli in 1915. It was dysentery and

associated diarrhœa, together with diseases of the enterica group, which at once claimed attention on arrival in Egypt and in Mudros that summer, among troops stationed there, and particularly among the men invalided back from the Gallipoli Peninsula. And on reaching that war area one came to realize the magnitude and intensity of the epidemic of intestinal infections which prevailed on each of our three portions, Cape Helles, Anzac, and Suvla Bay, during the summer and autumn of 1915, which was causing men to be poured off on to hospital ships by the hundred, sometimes by the thousand, every week, and which was mainly epidemic dysentery.

No other outbreak of dysentery at all comparable in magnitude has, fortunately, since occurred in any of the other Eastern war areas. Salonica has been far more satisfactory in this respect; dysentery was introduced into the Salonica force at, or soon after, the original landing towards the end of 1915, and some local prevalence resulted, which soon disappeared in the winter months. A recrudescence appeared in certain units in May, 1916, but the incidence was not great, and generally I understand that the prevalence of this disease in Macedonia has rather resembled the low level hitherto attained in France than that met with in the other Eastern war areas. In Egypt, through 1915 and 1916, there were a number of local outbreaks of dysentery, though few of any great severity, together with a general sporadic prevalence which only lessened in the winter months, November to March. In Mesopotamia we learnt, on arrival in August, 1916, that there had been much epidemic dysentery in the earlier part of that year, both among the troops taking part in the operations for the relief of Kut and at Basra and other base areas. During the time I was in Mesopotamia, in the latter part of that year, the dysentery record was, however, relatively good; there was a steady sporadic incidence of cases (represented by from 0·1 to 0·2 admissions, originally reported as dysentery, per 1,000 of the total force each week) and no very severe local epidemics.

I should have been glad to illustrate these very vague statements about dysentery prevalence by a series of figures and curves for the different fronts, but although this could be done with some confidence for Egypt in 1916, and for the latter part of that year in Mesopotamia, the series would still be too incomplete (particularly as regards Gallipoli and Mesopotamia in 1915) to be satisfactory, and would require too many reservations. It would also entail a use of figures as to the strength of the forces which might not be considered advisable even

at the present time. It is well, however, to give some indication of seasonal prevalence. That at Gallipoli, beginning at various dates after the different landings, reached a maximum in September, and fell very sharply at the end of October. No very pronounced seasonal curve was observed in Mesopotamia up to December last. In Egypt, as already said, the period of low prevalence was the cooler months, December to March, a somewhat abrupt rise taking place in the curve in March and a corresponding fall at the end of the year. I am not one of those who find an explanation for all dysentery infection in the activities of the fly, much as I have seen of the massive attack which these insects can make on the soldier, and on his latrines and his food if they are accessible. But I am certainly disposed to agree with those who attribute the seasonal rises and falls which I have mentioned to the increase or diminution of fly activity. The correspondence was close; no other explanation fits the facts more nearly; and the explanation is in itself almost self evident to those on the spot. I think the term "fly activity" preferable in this connexion to "fly prevalence." Towards the end of the summer, in the hot climates in question, flies may be extraordinarily abundant, but they are often in a state of torpor. They just hang in one place, dropping down to their nearest meal, with none of the spirit of adventure which gives them their infecting ability in the earlier months.

While referring to dysentery statistics, it may be useful to point out that their comparability may be seriously impaired by different customs in the use of the term. This not only arises in times of stress, such as at Gallipoli, which do not favour accurate observation and diagnosis, but also may result from administrative practice. Naturally if, in pursuance of a general order, every case, once it is labelled dysentery, is sent overseas, or if, to take another illustration, no case is to be termed dysentery without a laboratory diagnosis, there will be inevitable artificial restrictions or enlargements of the term, to the despair of the ordinary statistician. The practice was finally adopted in Mesopotamia of applying the term dysentery to every case with symptoms of entero-colitis in which blood and mucus had been found, provided that the fact of passing blood and mucus had been ascertained by inspection of the stools. This purely clinical interpretation, which I believe is now generally adopted, seems the one most likely to give useful results. It recognizes that in dysentery, as in other infectious diseases, there are cases of all degrees of severity, that some are quite capable of being cured locally, and that no one hard and fast administrative line need

necessarily be applied to the whole group. The argument that it increases the number of cases and so, to a superficial observer, "looks bad" for the health record of the force, is not one that ought to weigh; if it is given weight it may be counterbalanced by the correspondingly low rate of case mortality which can be adduced.

So far I have spoken of this disease as an entity—as of a single malady to be thought of, epidemiologically, as just "dysentery." The proceeding at first sight will seem out of date, when account is taken of the distinct specific causes of the clinical condition above defined—the Shiga and Flexner groups of bacilli, the *Entamœba histolytica*, and more rarely certain other protozoa of facultative pathogenicity. It has however substantial justification when one considers the broad results of the mass of work on the subject which has been carried out at base and field laboratories in the examination of the dejecta of dysentery cases. I have not heard of a single outbreak or local prevalence of dysentery being found to be associated practically with Shiga's bacillus alone, or with Flexner's bacillus alone. Always the laboratory experience has been the same—figures showing no doubt that one or another of these groups has predominated at a particular time or place, but invariably a substantial mixture of both. Not only so, but usually there were other associated cases in which various organisms of the same main group were recovered from the fæces, that in respect of sugar or agglutination reactions conformed neither to the Shiga nor to the Flexner organism, and whose pathogenicity is insufficiently known.

It is in this connexion interesting to note that in France the recent bacteriological findings also indicate the simultaneous operation during dysentery prevalence of a number of different bacterial types—in other words, that a local outbreak of dysentery is to be referred to a plurality of original infecting cases or carriers of different sorts, and not merely to the original chance introduction of infection by some one case or carrier.

But, it will be said, amœbic dysentery at least is quite another matter; the origin and spread of a protozoal infection is on a different plane to bacillary infection, and its epidemics should surely in some cases be distinct. This was certainly what I expected to find when going out in 1915, knowing very little of dysentery on the practical side. And at first it seemed true as regards Gallipoli. Many observations were made as soon as the dysentery cases reached Mudros hospitals, and others on the Peninsula itself, or as men were being passed on to the hospital ships, from which one gathered on good

pathological authority that free amœbæ in the greatest abundance were being found, not only in the freshly passed mucus of case after case of undoubted dysentery, but also in the fæces of many cases merely classified as diarrhœa. Admittedly the discrimination between the pathogenic *Entamœba histolytica* and the saprophytic *Entamœba coli* was almost impossible in the free amœbic form, and it had not been seriously attempted. But when the histological findings pointed to an intense epidemic, characterized by the presence of an abundance of amœbæ in practically every recent case examined, this seemed of little consequence. Moreover, certain fatal cases had presented the post-mortem characteristics of amœbic dysentery, and the cysts of *Entamœba histolytica* had been found in the stools of many cases from the Peninsula which reached Alexandria. As a working hypothesis, at any rate, here were grounds for regarding the whole outbreak as one of acute amœbic dysentery in which amœbic infection had been originally contracted by the troops whilst waiting in Egypt, and had spread wholesale and rapidly under the conditions met with in Gallipoli; with my colleagues I signed one or two reports in 1915 in which this hypothesis was fully accepted. In view of subsequent experience I do not now think this was a correct view of the case. One understands now that the disintegration of tissue and lymphocytosis of acute bacillary dysentery may produce mucus with macrophages closely resembling free amœbæ, and that when towards the end of the year more thorough bacteriological examinations were made of dysentery cases from the Gallipoli Peninsula, dysentery bacilli were freely found. In the many epidemics which have been studied since, in Egypt, Mesopotamia and elsewhere, there has not been one which showed a preponderating acute amœbic infection. Moreover, the extensive experimental work subsequently carried out in Egypt, besides that done for the Medical Research Committee and in other laboratories at home, has showed no such excess of chronic carriers of *Entamœba histolytica* among dysentery convalescents from the Gallipoli Peninsula as might have been expected if the nature of the Gallipoli outbreak had been so dominantly amœbic as was supposed.

In subsequent epidemics in other areas in which good protozoological work has been carried out and the matter tested by search for and identification of cysts (to which as a rule the protozoologist in search of amœbic infection limits himself), it has practically always been found that a substantial proportion of dysentery cases have yielded evidence of *Entamœba histolytica*, but always a minority—seldom over 20 per cent.

The significance of this figure is however largely discounted by the discovery that cysts are to be found in a large proportion of men not known to have suffered from dysentery at all. This was fully shown in Egypt, where Wenyon and O'Connor found over 5 per cent. of healthy men to show histolytica cysts in a single sample of fæces, and it has been confirmed elsewhere for men returning from the East. Histolytica cysts have even been found in healthy men who have never been farther than France, and (to judge by the recent Liverpool reports) in some who have never been outside England.

With the new work on carrier cases in mind, and with the laboratory data afterwards available in Egypt and in Mesopotamia, one thus comes to regard amœbic dysentery rather in the light of a concomitant of an epidemic due to other infections, highly important to know of for purposes of treatment and in the interests of the individual sufferer, but still always subsidiary to the main infections which produced the outbreak. The presumption was that men in these Eastern campaigns frequently ingest histolytica cysts, and give a lodgment to the resulting amœbæ which will at most produce a passive ulceration and seldom enough destruction of tissue to cause any very definite symptoms—unless or until there supervenes an acute intestinal disturbance, by irritating food, by food poisoning, by any of the ordinary causes of diarrhœa, or still more by a Flexner, Shiga, or other bacillary dysentery infection. When this happens, a definite amœbic ulceration may follow, with all its clinical consequences. The disease may then, in the absence of effective treatment, run its usual protracted course, the effects of the amœbic ulceration in such cases replacing or overshadowing those of the original bacillary infection or the initial irritation which brought about these results.

From such a view as this—and I believe it is after all quite an old one—it must by no means be deduced that the prevention of amœbic infection is of subsidiary importance; no one who has become familiar with the havoc on men's activities which may be wrought by chronic amœbic dysentery—minimized as it is by modern successful methods of administering emetine—could encourage such a belief. On the matter of prevention, however, there are fortunately few directions in which amœbic and bacillary dysentery differ. The most important is probably their liability to transmission by water under field conditions: the ordinary chlorination of water supplies liable to fæcal contamination, such as one part of free chlorine per million for half an hour, which suffices to kill the bacilli, has no effect upon the histolytica cyst, which

has been shown to be resistant to a ten-fold strength. I do not mean by this to lay too much stress on water carriage of dysentery infection. I believe it operated in Gallipoli and occasionally in Mesopotamia, but it was certainly only a subsidiary cause. All the evidence points to a very short viability of the dysentery bacillus in water—twenty-four to forty-eight hours at most—and in this it contrasts with enteric fever. On the other hand the cysts, like bacillary infection, have been shown to be carried freely by flies, and they are obviously liable to be spread, like bacillary infection, by all or any means which entail the direct or indirect contamination of food or drink, or of men's fingers, by fæcal matter, or even by its being swallowed when dried as dust.

One always comes back to this, that the prevention of dysentery, all dysentery, first and foremost is a question of practical field sanitation in its widest and most knowledgeable sense, measures which ensure that fæcal matter is at once burnt or deposited in fly and wind-proof pits or otherwise immediately and safely disposed of; measures for the detection of places which breed out flies, and for their capture; measures for the safety of drinking water, and the cleanly preparation and storage of food, the precautions at the cookhouse and messing places and so on. What this means, in infinite care and attention to details, often apparently trivial, by specialist, medical, and combatant officers and other ranks cannot here be specified; one can only pay a tribute of admiration to the results in the many places where these things were thought of and done to the greatest extent circumstances allowed, often under the greatest difficulties, discouragement and danger, alike in base areas, on lines of communication, and in the advanced lines, which often had better work to show in these directions than the base.

The association of epidemics of dysentery or of continued dysentery with failure to attend to these matters, from whatever cause it arose, was too close to escape even the most casual observation; one could give illustrations readily enough from Gallipoli, from West Mudros, and other areas.

Irritating and badly cooked food no doubt predispose to dysentery, and prompt treatment of any intestinal irritation is an important matter in the prevention of this disease. The Medical Advisory Committee emphasized in more than one of its reports that a timely dose of castor oil is one of the most essential prophylactic measures.

Another measure of the greatest value has been found to be the temporary removal at the earliest time of any diarrhœa cases, especially

if they be men who have anything to do with the handling of food; their treatment in field ambulances or, in times of an epidemic, in a hospital unit specialized for the purpose.

Finally, there are the measures for the detection of carrier cases and preventing their return until the carrier condition is cured. This measure, whether directed to bacillary or to amœbic carriers, must of course be reasonably subordinated to military requirements. Its utility probably lies mainly in its excluding men from return to active service who, though apparently convalescent, are still suffering from some intestinal disturbance and are very likely to break down again when exposed to field conditions. I think this is specially the case as regards amœbic infection. Much could be said for the view that in future no routine examination for cyst carriers is necessary except in cases where the man is himself out of condition or suffering from chronic intestinal irregularities. In such cases search for and cure of the amœbic condition is essential. No doubt also the introduction of infection is prevented to some extent, but it cannot be represented—certainly not on Eastern fronts, and from recent inquiry I think this is true also in France—that the security of the forces against dysentery depends on maintaining water-tight exclusion of carrier cases or of the convalescent dysentery cases which remain carriers. It is a good practical rule in field sanitation to regard every healthy man as a possible carrier and take precautions accordingly.

Enterica.

The general use for epidemiological purposes of the term enterica, to cover diseases produced by the typhoid and paratyphoid bacilli, may be justified on grounds similar to those in favour of the generic term dysentery. So considered, one may say that enterica was met with in all the campaigns under consideration, in circumstances roughly parallel to the occurrence of dysentery, but with some important differences. As a whole there were far fewer cases of enterica than cases of dysentery. Except in Gallipoli and Mudros in 1915, enterica was also less widely diffused, and there have been many regions and important units which have remained almost free from enterica cases, certainly from anything which could be called enterica prevalence. In this matter again the Macedonian force had a good record. Egypt, on the other hand, showed a considerable prevalence of enterica in 1915, and also in the earlier part of 1916, at a time when a large number of troops returned from Gallipoli

had reached Egypt. For Mesopotamia I have few data until the summer of 1916, when diagnosed enterica was of a low order of prevalence, contributing among the British troops a weekly rate of new admissions which was under 2 per 1,000 for the whole force, and falling steadily to 0·5 per 1,000 in December, while the corresponding Indian figures were considerably lower.

For purposes of ordinary preventive work in the field, and for stopping the spread of the disease when cases occur, it is of little consequence whether the enterica is typhoid, paratyphoid A, paratyphoid B, or combinations of these infections. In the matter of protective inoculation, however, the distinction is all important. In the Eastern areas, as no doubt elsewhere, one has learnt never to accept without close inquiry percentages of incidence of the different varieties of enterica, still less to compare those obtained by different methods. It is safer to judge from thorough sampling examinations at selected laboratories than to mass those from different laboratories, even when one excludes from consideration all serological tests and judges solely by cases in which the actual organism has been recovered, either from the blood or from the excreta.

Taking massed figures, however, the results from Gallipoli (1915), from Egypt (1916), from Salonica (January to May, 1916), and from Mesopotamia (August to December, 1916), were very much of the same order: of 100 enterica cases in which the organism was recovered the figure for true typhoid was about 20, that for paratyphoid A anything between 50 and 70, and for paratyphoid B 10 to 30.

It must not be assumed that these are proportions which hold good throughout; there were numerous cases for example in which a local prevalence has been almost wholly associated with paratyphoid A, without concomitant paratyphoid B, or typhoid. More rarely an outbreak almost exclusively paratyphoid B has been found—one such occurred in a section of the Eastern force in Egypt in 1916. As regards inoculation, I would here, without attempting figures, merely record the opinion, held I believe universally by all who had looked into the subject, that the anti-typhoid inoculation to which the great majority of the troops had already been subjected before reaching the Eastern areas and its systematic renewal is primarily responsible for the relatively small amount of true typhoid which prevailed. When one considers the conditions in Gallipoli at the time when so much infection of excretal origin was present, and was being manifested wholesale in dysentery and paratyphoid, it would be imperative, did one not know about inocu-

lation, to seek for some special explanation of the extraordinary relative immunity from true typhoid. Antityphoid inoculation however supplies that explanation. The same may be said of the other fronts where experience has proved that similar causes of infection were operative. Another noteworthy point is that true typhoid, when it has occurred and been verified by the recovery of the typhoid bacillus, has so often been mild or anomalous in type. I have only seen one large collection of true typhoid cases with the regular clinical history that one used to associate with typhoid: characteristic charts, relapses, perforation, early and late hæmorrhages, and so forth. This was in 1915 in the recently arrived New Zealand force, which, as is well-known, was subjected to inoculation in New Zealand with a strain of vaccine that was almost certainly inert, and produced no local or general reactions.

The large amount of paratyphoid infection in Gallipoli was one among many powerful arguments for the introduction of the present triple (T.A.B.) vaccine, inoculation with which became general in 1916. I am not in possession of data sufficiently recent to be able to speak of its protective effect against the paratyphoid infections, though I believe it to have been considerable. Figures of typhoid incidence before and after the general T.A.B. inoculation, worked out by Colonel Hunter and Major Willoughby in Egypt, certainly pointed strongly in this direction, and the whole evidence available after the war will be of the greatest interest. It was anticipated from a knowledge of the shorter duration of the serum reaction after attacks of paratyphoid A that the immunity conferred by a paratyphoid A vaccine might be slighter or shorter in duration than that conferred by the typhoid vaccine against typhoid. Several cases in Mesopotamia, in which men were attacked by definite paratyphoid A within one or two months of their inoculation with triple vaccine, pointed in this direction; it should however be added that the vaccine used at that time in Mesopotamia was, as regards paratyphoid A, only half the bacterial strength of that used at home and in Egypt.

Camp Jaundice.

Not so long ago I should have been disposed to class camp jaundice with dysentery and typhoid as a disease probably due to the same kind of causes, to insanitary conditions leading to food contamination, and the carriage of infection of excretal origin by flies or in other ways. One was led tentatively to this view in Gallipoli, when, during the autumn

of 1915, the general epidemic of sickness due to dysentery, enterica, and diarrhœa became complicated by a considerable and widespread prevalence of jaundice. In previous campaigns, epidemic jaundice has been associated with insanitary conditions of camps, while some observations made on the French forces at Cape Helles by Sarraillhé and others pointed to the conclusion that the jaundice there prevalent was a manifestation of infection by paratyphoid A or a similar organism of the enterica group. Since then it has been shown, I think, that this association was probably one of coincidence and not cause and effect.

As regards the British forces at Gallipoli at any rate it became clear that the jaundice and diarrhœal group of diseases were not running parallel, the former continuing to increase at a time when the diarrhœal group had undergone a marked decline. Since then I have been naturally on the look out for association of camp jaundice with dysentery and enterica, but have been able to detect little or no association between them.

On all the fronts one met with occasional sporadic jaundice, much as I presume one gets at home, but definite epidemics of jaundice were rare. A small outbreak occurred in Salonica in November, 1915, mainly in men of a division which had come from Gallipoli, and this continued during the cold months of December and January. A short outbreak was also noted in one division in Mesopotamia in course of the very hot weather in June, 1916, affecting some 550 cases, and lasting altogether about six weeks. This epidemic arose shortly after the division had returned from operations in the more forward area and was going through a period of relative rest with more abundant diet. The history was one of general and almost simultaneous occurrence and progress of jaundice throughout the divisional camp, in the same sort of proportion in all its constituent units. It suggested some generally operative cause, but did not point to case to case infection. Some other outbreaks occurred in Egypt, as to which I had little opportunity of inquiry, and I fear I remain as puzzled as before as to the explanation of these epidemics. None of them, so far as I am aware, comprised cases in which the jaundice was symptomatic of Weil's disease or the icterohæmorrhagic spirochætosis which has been described in France; spirochætes were searched for in jaundice cases in Mesopotamia, but without success. Even if in future it was to be shown that camp jaundice is associated with spirochætal infection, many special points of the ætiology of this disease, particularly the local limitations of its epidemics, would apparently still need to be explained.

Cholera.

It is remarkable that so little has been seen of cholera in all these great campaigns—for great each one has been by reference to all earlier war standards—campaigns which have brought our forces, including large Indian contingents, as well as a host of native labourers and followers, into contact with the Turk and the Arab in regions which have experienced sweeping epidemics of this disease in the past. In Gallipoli in 1915 cholera was one of the epidemics which was most anticipated, but it did not come. In Macedonia the cholera of the 1912 Balkan campaigns was a recent and disquieting memory. Ever since then cholera was believed to have lingered in the Balkans, at any rate in the neighbourhood of Adrianople, though its type was anomalous and possibly decadent. There was, however, no cholera among the allied troops of the Salonica force when they got into contact with Turks and Bulgars at the end of 1915, nor, so far as my information goes, has there been any since. Egypt for many years in peace time has strained every nerve to keep out cholera, especially by the regulation of the pilgrim traffic, so successfully that it is some fifteen years since that country has had more than an occasional sporadic importation. The pilgrimage was shut down during the war, but would not the enemy on the Eastern and Western fronts, or our own troops in transit from India and other endemic centres, bring us cholera infection, or at least import unsuspected cholera carriers with all their possible consequences? That there was solid ground for these apprehensions, and for the various precautions to which they gave rise, was shown by two small outbreaks in Egypt in 1916, one in a hospital unit to which infection appeared to have been brought by convalescents arriving from Mesopotamia, the other due to infection from Turkish sources incurred in course of operations east of the Suez Canal. These cases, not more than forty altogether, fortunately represented the whole story of cholera in Egypt from the beginning of the war to March of this year, and I believe the recent history has been equally good.

Mesopotamia in the present year has, I believe, had almost an equally satisfactory record of cholera. It was otherwise however in 1916, during which some 1,700 cholera cases (British and Indian) occurred in the Mesopotamia Force. The bulk of these cases occurred in an epidemic between April and June and I have little information

about them save that a portion at least was traced to infection of water obtained from a cutting off the Tigris, which remains on our maps labelled as "Cholera Creek," near which, at the time the epidemic arose, two Turkish prisoners suffering from cholera were captured.

In the months which followed, July to November, 1916, there was a continued sporadic incidence of cholera, on a relatively small scale, both at Basra and at various points along the Tigris. There must have been numerous carriers of the disease and the fact that it nowhere became widely and suddenly epidemic is, I believe, mainly to be attributed to the circumstance that water in nearly all cases was taken from the Tigris, and to chlorination. The volume of water in the Tigris was so great that any infection would be largely diluted; moreover for several months its muddy waters are full of suspended mineral matter, 90 per cent. of which subsides on twenty-four hours' standing. As regards chlorination, although some irregularities were inevitable when hundreds of small plants, mainly pits dug in the ground, revetted with sandbags and lined with tarpaulin, had to be separately treated, yet as a whole it provided systematic sterilization on a comprehensive scale, more often efficient than not, and steadily improving with experience and better material. One of the greatest difficulties was due to the variations in the chlorine content of different tins of bleaching powder, and particularly to its rapid loss of chlorine once the tin had been opened. A very useful advance was made when the Central Hygiene Laboratory took to supplying standardized concentrated solutions or suspensions mixed up by hand churns, which kept their strength. Later on, with the development of large water services at the base, various methods of admitting chlorine into the supply in amounts automatically proportionate to the flow became available.

How far the other fronts owed their escape to similar water precautions—as a rule marvellously thorough—how far to absence of infection, and how far to inoculation, is difficult if not impossible to say. While it is true that the existence of cholera among the Turkish forces opposed to us was on several occasions suspected and once or twice proved, I heard of little evidence of any widespread epidemic among them. In this connexion it was interesting to find from statements of prisoners in Mesopotamia, and particularly in Egypt, where a very careful inquiry was made on the point, that they had been inoculated against disease a great many times, and particularly often against cholera.

It is difficult to appraise the evidence which any of the campaigns afford in favour of inoculation against cholera. In Mesopotamia it was certainly the fact that the prevalence of cholera in the early part of 1916 occurred before the systematic inoculation of British and Indian troops with anti-cholera vaccine. The fall in cholera roughly coincided with increase in this inoculation, but at the same time it became possible to introduce vast improvements in other measures of preventing infection. In Gallipoli, Salonica and Egypt, anti-cholera inoculation was general practically from the beginning, going frequently in some units nearly to 100 per cent. standard, and was systematically renewed.

A question brought before our Committee at the outset in 1915, and often since, was whether cholera inoculation should be done straightway or should wait for the appearance or at least substantial threatening of the disease. The evidence of the short duration of protection—like all other evidence as to the protective value of cholera vaccine, mainly dependent on the Greek accounts of 1912—made this an important practical question. The Committee invariably advised systematic inoculation in advance of any known threatened infection, and its renewal from time to time, preferably at least every twelve months, and I believe on a balance of the arguments that this contention has been sound. In the Mesopotamia force a weaker vaccine had been used than elsewhere, and a trial of a stronger vaccine of 12,000 millions in the double dose was to be made.

Malaria.

The incidence of malaria on the various fronts, their anopheline history, the steps taken to combat the formidable epidemics which have occurred and to prevent those which have not, would require an address by itself, and I can only make a few passing observations here. Personally I missed the great historic epidemic of the war, the malaria which began suddenly in Macedonia in June, 1916, when the British forces, leaving their entrenched line north and east of Salonica, penetrated the Struma Valley and other baneful regions and held them against the Bulgars. Only one month before, when our Committee had left Salonica, the prospects of escape from serious malaria on the ground then occupied were quite good. The country behind the lines was even then a large one, but the question of malaria had received very close attention from the beginning. Areas specially swampy and liable to malaria had been worked out, some sections had been efficiently

drained, while quinine prophylaxis for the troops and certain native populations and other measures were well in hand. There were weak points in the defence and a rise of the malaria curve was anticipated, but there was every reason to hope that it would produce no large amount of sickness. As is well known, the advance necessitated by the military situation in June changed all that, especially in the Struma Valley. Here the regular incidence of malaria among the inhabitants of the native villages was even more virulent than usual and causing a notable mortality. It is a broad plain with luxurious vegetation and endless marshy hollows. Mosquitoes swarmed, the chief vectors of malaria being apparently *Anopheles superpictus* and *maculipennis*. A man's daily bites were not to be counted by units but by the score; malaria spread with great rapidity; the weekly evacuations for a time ran into four figures, base hospitals and hospital ships filled, and cases began to pour into Malta and many presently to be sent home. The epidemic on these fronts declined in the autumn, but never wholly subsided. I understand that apart from many relapses new cases occurred in the winter months, some among men who had only recently come out from home. If so, the experience was similar to that met with in Basra and Mesopotamia, where the common notion, that after a certain season is over risks of malaria cease, had to be considerably modified. There was always a residuum of adult, already infected, anophelines ready to emerge on a warm day, especially from tents and bivouacs which were not opened up, and to bite vigorously even in January: the curve of freshly infected cases was flattened but not extinguished.

With only partial hearsay knowledge I do not attempt to specify the nature of the efforts which have been made in Macedonia to combat the malaria danger, especially in the present year, by changes of ground where practicable, alterations of clothing, netting on a scale never before reached, quininization of carriers, and so on. One knows enough to realize that the complete story of the fight against malaria in Macedonia, when it comes to be written, will form an epic full of fine work and no little self-sacrifice, which has at least partly had its reward by the later improvements of the malaria record.

The epidemic malaria with which one met in Mesopotamia in 1916-17 was fortunately on a much smaller relative scale. From Kut to near Kurna—and I imagine the same to be true between Kut and Bagdad—the bare Tigris plain will not as a rule support even the mosquito. In the few areas where water is run off the river for

irrigation it is quickly absorbed or evaporated; there are few channels of stagnant water and few or no marsh areas. It is otherwise when you descend to the junction of the Tigris with the old channel of the Euphrates and reach Kurna, where the date palm groves begin to be abundant on each side of the river. I could never ascertain why—save for a venerated but disreputable plum tree—Kurna was credited as the Garden of Eden, but if that garden was there, our first parents needed no other excuse than the mosquito for seeking the quickest available body covering and leaving the district. All the way down past Basra to the mouth are also irrigated palm areas which would permit an abundant mosquito-breeding if it were not for the tidal oscillations in the creeks and channels, which keep it partly in check. In the summer of 1916 it was noteworthy that the malaria met with in all parts of the Mesopotamian area could most often be traced back to an original infection contracted in the Basra to Kurna zone.

The reported new cases between August and October were usually from 2 to 4 per 1,000 (British and Indian alike) each week, while much escaped notification through the unfamiliarity of the medical staff—mostly men who had still to gain their tropical experience—with the clinical results which malaria infection may produce. No doubt in 1916 malaria contributed considerably to the mass of miscellaneous sickness which was recorded as due to the "effects of heat" or even to heat stroke.

In Egypt the Suez Canal region needed and received close expert attention in consequence of the existence of large areas in which *Cellia pharoensis* and other anopheline carriers in Egypt, bred out, particularly certain areas irrigated or swamped by blind outflows from the Sweet-water Canal. Certain of the occupied oases in the south and west were also notorious for malaria. Its greatest prevalence in these regions appears to have occurred in the autumn. Relatively to the forces concerned the extent of malaria has been slight, a fact largely to be attributed to the attention which has been given to anti-malaria measures.

Personally I was throughout in the position of having to learn the rudiments of malaria prophylaxis from those who had made the subject a life's work, and was fortunate indeed in having the tuition of my colleague, Lieutenant-Colonel Andrew Balfour. Like many others I had to learn that anti-mosquito work does not consist in merely sending about a few men with spades and an oil drum; it must be done thoroughly by regular and adequate labour units if it is to be of

any use, and supervised by someone who can think on the ground in terms of the mosquito.

Save in a few places, such as the areas near Salonica, or special regions on the Suez Canal, the exigencies of the campaign usually put the efficient destruction of breeding grounds out of practical possibilities. One had to rely on measures for preventing the mosquito bite; for the removal, or at least the thorough cinchonization, of human carriers of the malaria parasite, and on prophylactic doses of quinine for the healthy men. The latter is a most difficult and controversial subject, facts on which I hope will later on be contributed to this Section. One of the best statements of the case for quinine prophylaxis, from Panama experience, had, in 1914, been given by Craig in the U.S. War Department Bulletin, and the importance and value of this prophylaxis had strong support from many other authorities. Our Committee advised its adoption during the malarial season in troops specially exposed to risk, where possible in doses of 6 gr. daily. That there should have been such intense malaria in Macedonia, notwithstanding the use of quinine by the ton for this purpose, has been disappointing, and has led to doubt being expressed on the soundness of the practice, and also to further investigations—like those recently reported by Lieutenant-Colonel James—of the stages at which the parasite becomes quinine resistant. The objections raised are not wholly removed by evidence that the prophylactic administration of quinine has been haphazard, irregular, or in insufficient dosage. There is plenty of such evidence. Sitting at home one might well think that in a body of men under military discipline nothing should be more easy, save in periods of active engagements, than to secure that they take a given tabloid or draught at a given time once a day; one has only to see the problem in the field to realize its great practical difficulties. All the same one cannot get away from the facts of heavy incidence of malaria on troops in which the prophylactic administration was well carried out, no doubt in areas where the dose of infection was large and frequently repeated. One has to balance between the view that the incidence would have been still heavier but for the prophylactic, and the conclusion that the prophylactic use of quinine is but a broken reed to lean upon.

I can only say here that on such evidence as I have been able to study I think that the preventive use of quinine for healthy troops in malarial areas in such doses as those mentioned, was, and is, a right measure to adopt, though I trust that from the large amount of work

which is now being done on the subject, both at home and in the military laboratories abroad, we will soon have some decisive and conclusive evidence as to the best method of procedure in this respect.

Trench Fever and Louse-borne Diseases.

When the Medical Advisory Committee were at Salonica in May, 1916, trench fever had recently been discovered. A great number of cases of pyrexia of uncertain duration and accompanied usually by great tenderness or pain in the shins, unbearable at night, had come to notice during the previous four or five months. They had been closely studied, particularly by Major Hurst, R.A.M.C., who was at that time a consulting physician to the Salonica force, and it had been recognized that most of the cases which were being diagnosed as pyrexia of uncertain origin had clinical features in common. A kind of agreement had been arrived at to describe certain types of this pyrexia of uncertain origin as P.U.O. (*a*), (*b*), &c., but the arrival of papers by McNee, Renshaw and Brunt[1] showed that in France these types had been identified, and were being described as varieties of a single epidemic disease, "trench fever." It appeared moreover that the disease, under the name of "gaiter pain fever," was known to the Austrians, among whom it had been epidemic in Galicia, and there could be no doubt that the Salonica force had been suffering from the same disease in its various forms. Here, as in France, all gradations were met with between the short or acute type—a sharp fever of about a week, usually with considerable tibial pains, with at most one or two secondary and less severe relapses, and the long or chronic form in which very regular recurrences of fever were met with over many weeks.

A revision of the entries of "P.U.O." in the books of field ambulances and base hospitals showed that the disease had occurred in December, 1915, in troops of a division coming direct to Salonica from France; that by May all the divisions in the force had been affected to a greater or less extent, the forces at the front relatively considerably more than those in the base areas. Individual attacks were as a rule mild enough, but the total effect of so much temporary invaliding on the efficiency of the force was considerable, and in a minority of individual cases recovery was very protracted. Many battalions and units had only had a few cases altogether, but some had

[1] Cf. *Journ. R.A.M.C.*, April, 1916, pp. 490-524.

been severely attacked, the epidemic usually spreading rapidly for the first two or three weeks in the affected unit, and afterwards being followed by a dropping fire of cases.

It was of course a large assumption that all these cases diagnosed as "P.U.O." had been in fact trench fever, but I think it must have been very close to the truth. With the definite recognition of trench fever it became possible to get more exact information and largely to discard the term P.U.O. It is a term which in any case should seldom or never be used in field ambulances which keep their cases only a short time; undiagnosed cases may be sent down as "fever undiagnosed," but the term "pyrexia of uncertain origin," which is accepted as a final diagnosis, is rather one for base hospitals when they have exhausted all their resources of identification. Our hopes at Salonica that closer ætiological study could be made of the disease were frustrated by its rapid diminution after May and by the all-compelling demands of the malaria epidemic. I have not been able to ascertain the extent to which it has been prevalent again in the present year, but certainly one has heard little of it.

The outbreak to which I have referred was from eighteen months to two years ago, and since then much has been done in France to study its ætiology and method of spread. We have learnt that the infection is inoculable and is contained in the corpuscular elements of the blood and have had theories of the disease as a spirochætosis.

The whole matter at the present time stands referred to a special investigating committee in France, whose information is far more recent and comprehensive than anything that was obtainable at Salonica in the early days of its recognition. The louse was there suspected as carrying the infection, and I believe the possibility of transmission by the louse has been demonstrated more or less convincingly by actual experiments. In Salonica it seemed that if any blood-sucking insect was concerned it was in all probability the louse, as in December and January there were few biting flies, midges or mosquitoes. A difficulty, however, was caused by the many attacks of officers and others who had no signs of lousiness. This can of course readily be explained by the assumption that one or two casual lice, to the bites of which anyone may have been exposed without knowing it, are enough to carry the infection. But on such an assumption one would have expected many more multiple cases of trench fever in the same dug-outs or bivouac than in fact was the case. A few pronounced groups of cases were met with, but hardly more than would have occurred as the result of chance

from a general cause common to everyone. As a rule, when a unit was attacked, even when severely attacked, it was rare for more than one man to be affected in a group of men in the same bivouac. Individual susceptibility might then be invoked, but it was difficult to make much assumption of a rare susceptibility when the heavy incidence in certain units was considered. It was, moreover, somewhat surprising that this disease, if louse borne, is not known to have accompanied typhus and relapsing fever. One may hope that these and other difficulties in interpreting the spread of trench fever on a louse-carrying hypothesis may be met by further knowledge.

There is, as far as I am aware, no better hypothesis in the field. The suggestion of rat infection, which I believe has been made, would not have been supported in Salonica, where rats were few. There was very little accompanying catarrh, or indication of spread by respiratory infection. As a practical question, on present knowledge, the active employment of measures for delousing, together with the early detection and removal of cases, seem to be the chief measures of prevention.

Having learnt something of trench fever in Salonica, I naturally inquired for evidence of its existence in subsequent visits to Mesopotamia and to Egypt in 1916 and 1917. One or two suspected cases were spoken of, but the disease, if present at all, had certainly shown none of the rapid spread in epidemic form which was characteristic of it in Macedonia, though lice were common enough.

On all these fronts the question of delousing troops as a protection against relapsing fever and typhus was constantly arising. Typhus, happily, has been practically non-existent; relapsing fever occurred among Indian troops in Gallipoli and Egypt and very slightly in Mesopotamia. In Egypt relapsing fever is a common disease of the native population, especially in the cooler months of the year, and in 1916 a formidable epidemic occurred among the Egyptian labour corps, which attained its maximum in April and thereafter rapidly declined. Only some 200 British troops were attacked. Here isolation and rigid measures of disinfection usually sufficed to keep relapsing fever within small compass. It is noteworthy that few of many panaceas against lice which were put out at the beginning of the war have stood the test of time and practical experience. Nearly always one came back to current steam for clothing and blankets, or to cresol or petrol emulsions. For delousing by steam one needs to get away from prepossessions in favour of elaborate disinfecting

apparatus or even the ubiquitous "portable Thresh." Current steam can be used much more efficiently, expeditiously and economically in improvised apparatus of a much simpler kind. The railway steam disinfecting vans introduced in the Suez Canal area at the instance of Colonel Hunter are examples in point, enabling the kits of several hundreds of men to be disinfected at one time by turning on steam from the locomotive which takes about the vans. The vans have been of incalculable value in preventing the spread of relapsing fever in Egypt.

One learnt early that effective delousing of a unit is one of the most troublesome duties that a medical officer can undertake: it is not that it is usually difficult in itself, but it needs a thoroughness and devotion to detail, and endless trouble to secure the co-operation of combatant and other officers which many a beginner does not realize, and from which many an old hand recoils. But the work is well worth doing and persisting in, and apart from removing anxiety as to typhus and relapsing fever, and possibly trench fever, it has its reward in freeing men from what to many is a very real source of discomfort and loss of self respect.

Sandfly Fever.

The sandfly was troublesome in Gallipoli in the autumn of 1915; at Cape Helles much more so to the French who inhabited or used old stone buildings in Sed-el-Bahr, than to our own troops living in recently made dug-outs or, in the case of hospitals, under canvas. True sandfly or pappataci fever was conspicuously epidemic in the late summer on the French side. Though our own troops were troubled with sandflies here and in Egypt (I have no facts as to Salonica), I only came thoroughly to realize sandfly fever when I met with it in abundance in Mesopotamia in August and September of the following year. At that time sandfly fever, and "P.U.O.," due very largely to sandfly fever, were keeping up the sickness-rate, and accounting for about one-third of the total sickness from all causes in the British troops. Its period of epidemicity was from July to October, and it often spread with great rapidity through the units affected. The actual duration of the fever is usually only a few days, and it had of itself few after-consequences besides those of muscle fatigue and exhaustion, but the epidemic, nevertheless, had considerable military importance. At a critical time of reconstruction and reorganization men were constantly and most

inconveniently being put on the sick list, and the attack coming at the end of the trying hot weather seemed, like all otherwise trivial maladies at that time, to be met by little or no resistance.

There was little to be done in the way of prophylaxis except to try to keep off the ubiquitous sandfly. Mosquito nets, if under eighteen holes to the linear inch, were often little more than traps for the sandfly, which could be found in the morning fully gorged along the top edges and in the corners of the net. On the other hand, a fine mosquito net, twenty-two holes to the inch or more, was really effective at night, though the favourite biting hours of the sandfly were often at the end of the day when nets could not be used and one had to trust to repellents. Of these experience seemed most to favour smearing the exposed surfaces with vermijelli, which was available as an issue for louse destruction. It was more lasting than the volatile oils, and needed renewing only once or twice in an evening.

Bilharzia.

Bilharzia is a disease with which ordinary workers at preventive medicine in England are little concerned, but one naturally read and inquired about it on reaching Egypt where it is so common. A severe bilharzia infection is a serious and crippling disease, with its accompaniments of hæmaturia and painful micturition due to the constant passage of the ova through the bladder wall or of the results of rectal bilharzia, and it was the more to be apprehended as several months are required after infection for symptoms to develop. The text-books at the time carried one to the stage of realizing that the infection once acquired is very rarely cured; its results could only be palliated, and even with treatment the soldier would often be deprived of his fighting efficiency. They gave a life history of the parent worm in the human body, which inhabits the blood stream and gives off its ova, and added, merely as a conjecture, that outside the body the embryo went through a further cycle, probably in the water snail.

In Egypt in 1915, however, the Bilharzia Mission sent out by the War Office had already succeeded in carrying knowledge a stage further, and had demonstrated that at least two quite distinct freshwater molluscs of the genera Planorbis and Bulinus were the intermediate hosts of the parasite; and further, that the ova, when ingested by these molluscs, go through various stages resulting in the emergence of free swimming cercariæ which subsequently gain entrance to the

human body by being swallowed or by piercing the skin. These researches gave the opportunity of a sound system of prophylaxis based on ascertained facts, such as that the degree of risk of a given source of fresh water can be established by search for the implicated molluscs, that the cercariæ, though they can work through a sand filter, can only survive in water for about forty-eight hours after leaving the snail host; that while practically unaffected by bactericidal doses of free chlorine, and by many other dilute disinfectants, cresol in quantities such as 1 oz. to 60 gal. destroys the parasite rapidly. The system promptly established on such facts was deservedly successful, and bilharziosis has not occurred as a serious epidemic among the troops. It consisted essentially in securing adequate delay of the water before drinking in places like the Suez Canal zone, which are dependent on the so-called Sweetwater Canal, or similar long channels from the Nile, and in inhibiting all fresh-water bathing except in special pools where the water was duly treated with cresol. However laudable the motive, no inhibition on bathing or drinking was likely to be a popular measure in a hot and thirsty land, and it speaks well for the organization that it has been so successfully and thoroughly insisted on. The rare cases have been traced back to neglect of the rules with much nicety: in one case it was possible from the description given by a group of soldiers suffering from bilharzia in a base hospital to go straight to the pool which they had frequented months before, there to find an abundance of the snail hosts, some of which contained the parasite. The foresight displayed in regard to this disease, and the results obtained, must be regarded as one of the most gratifying chapters in the preventive medicine of the war.

Leishmania.

The Oriental sore which results from leishmania infection in Mesopotamia often goes by the name of "Bagdad sore" on account of its frequency in that city. As you are aware, the complaint is often very intractable notwithstanding various modern treatment, such as intravenous injections of tartar emetic, and it leaves most disfiguring scars on the face and other exposed surfaces which are usually attacked.

When the outskirts of Kut and the Shatt-el-Hai formed our furthest western limit, it was noticeable that the Arabs, adults and children, who bore the characteristic scars, had usually come from higher up the river,

and at one time it was thought that no active leishmania infection went on in Lower Mesopotamia among the Arabs or was to be found among our own troops. This view had, however, to be corrected on the discovery made or confirmed by Lieutenant-Colonel Wenyon of several isolated cases or small groups among Gurkhas or other Indian troops at the end of 1916.

I have no information from Mesopotamia since Bagdad was occupied to gauge the extent to which our forces have since been attacked; one would expect a material increase.

Apart from early treatment, and the separation of known cases, or at least keeping the sores covered, no special preventive measures seem applicable. The sandfly is suspected as a carrier, though it is difficult to imagine that there can be greater prevalence of sandflies in Bagdad than in many places in Lower Mesopotamia. In any case, as I have already said, the prevention of sandfly bites is no easy matter. The forces in Egypt and the Mediterranean have, I believe, been free from leishmania. It is noteworthy that bed-bugs, which have been suspected as carriers, are among the few insect pests which do not seem to thrive in Lower Mesopotamia.

Diseases Due to Food Deficiencies.

In the Eastern campaigns there have, of course, often been far greater difficulties in rationing and feeding troops than have occurred in France and Flanders. It is marvellous, not that failure of supplies has sometimes occurred, but that it has occurred so seldom. The good feeding of the troops as a whole must have contributed enormously to protect them from disease and enabled them to resist disease. Earlier in this paper I have spoken of the almost complete absence of typhus and the way in which relapsing fever, though its infection was often introduced, has been kept within very small limits. The good standard of nutrition has possibly had much to do with this in addition to the measures for suppression of the louse. As regards the medical service, it is remarkable on looking back that I do not recollect a single hospital or field ambulance—and I have visited hundreds—at which there was any shortage of the essential foods required.

At Anzac, in September, 1915, the whole force had for a long time suffered, not strictly from any actual shortage of foodstuffs, but from months of monotonous diet represented by the staple corned beef, biscuits and tea. Up to that time the supply of bread had been very

scanty; fats, jams, &c., were decidedly rare, though these were more available later. Men in many instances had a distaste for the food they had, sometimes from bad teeth, and often from the absence of anything like pickles or condiments.

There was no doubt that the dietary in many cases had produced bad nutrition and in not a few cases this was associated with pronounced cardiac weakness. The forces at Suvla Bay, which had landed later, were more fortunate in this respect. I could not ascertain on any of the occupied areas of the Gallipoli Peninsula that cases had been evacuated suspected to be suffering from beriberi, but a few cases from the Peninsula which reached hospitals at Mudros were diagnosed and described by Colonel Willcox as due to this disease. Some scurvy appeared among Indian troops in Gallipoli, doubtless due to the same causes as in Mesopotamia, where again evidence of the result of diet deficiency came to notice.

In Mesopotamia the river transport and the hot weather in a measure governed everything, and during the period which followed the surrender of Kut a period supervened in which these obstacles were at their greatest. The British troops as a whole, however, suffered to no very serious extent in health from the restrictions in food and monotony of ration. In the year before, 1915, I understand that such conditions, greatly accentuated, had produced some beriberi among them; in 1916, however, the extent and duration of any anti-beriberi vitamine deficiency was fortunately not sufficiently great, and there were practically no cases of this disease. Had it been more prolonged, a powerful preventive would have been available in the form of compressed yeast tablets or "marmite," which in view of the 1915 reports had been sent out from home. The only cases of suspected beriberi which came to the notice of the Medical Advisory Committee was a group of some sixty cases of little severity at a camp of British troops near to Basra in November, 1916, the identity of which with real beriberi was open to some doubt, both clinically and epidemiologically. In this outbreak new drafts were affected in some instances within a very few weeks of their arrival in Mesopotamia, the diet at the camp having been liberal and varied, and in some of the cases having frequently included eggs.

The Indian troops in Mesopotamia, however much exposed to deficiencies or monotony in diet, wholly escaped beriberi, the explanation probably being that their stock cereal, atta, contains a good proportion of germ, and also that some anti-beriberi vitamine is contained in the split leguminous dried peas or beans issued to

them as dhal. In regard to scurvy, however, it is very different. Very few scurvy cases occurred among the British troops. Their rations, though as deficient as the Indian in fresh vegetables, included much more fresh meat. Moreover, there was no question of general diminished nutrition; they used canned and preserved foods freely, and in this way were better off as regards nourishing foods than the Indian troops. The latter were dependent on cooking every meal, and this in turn depended on a sufficiency of fuel in a land where every stick had to be brought up, possibly a hundred miles or more from the base. They included men who greatly objected to eat meat if available, either because of caste, or because of pyorrhœa which made meat eating painful. From such causes Indian troops suffered severely from scurvy—not, it should be noted, throughout Mesopotamia, as might be inferred from the observations on scurvy made in the Mesopotamia Commission's report, but in those areas of the Tigris where the difficulty of sending up accessory food supplies, meat, and fuel, was greatest.

At the base, and also on the Euphrates front, where fresh vegetables were obtainable, there was practically no scurvy save in one or two units, for which a special explanation was forthcoming. On the Tigris front it was interesting to find that of three separate and comparable Indian forces the smallest incidence of scurvy relative to strength between August and October was on the force the shortest distance from supplies, and the greatest on the force most difficult of access, while the middle figure for scurvy was that on the force midway. The epidemic on this front was of considerable magnitude, some 7,000 cases being reported, and it was serious in its results. During four months, July to October, the evacuation figures from scurvy in the Indian troops at the front was occasionally as high as 4 per cent. of the force in a week, and it was seldom below 1 per cent. The epidemic ceased only with the onset of cooler weather, the improvement of transport, and increase of supplies. Fresh vegetables then became to some extent available, and efforts were successfully made to secure greater supplies of fresh meat for the Indian troops. By the end of the year also much better issues of fresh vegetables were made right up to the front line. The advance to Bagdad happily brought the troops, during the last hot season, within reach of a large cultivated region, and for this and other causes there has been no repetition this year of the 1916 experience.

The Indian scurvy epidemic of 1916 will no doubt form a landmark

in the history of scurvy, and has already given a new and important impetus to its experimental study—for example, the use of the anti-scorbutic properties of dried legumes, which have been allowed to germinate before cooking. This method, fully worked out by Miss Chick, is of really great importance and it is possible that before the end of the war occasions may arise in which its adoption will be invaluable. Did time permit one would be glad to enlarge on some of the main features of the outbreak. It seemed fairly clear that during the scurvy-producing period those who escaped were getting their chief antiscorbutic from the fresh meat which was available, and from the occasional onions, which were about the only vegetable to be had. Various fascinating figures were collected as to the incidence of the disease on men who were meat eaters and those who were not, but they were greatly disturbed by the proportion of meat eaters who had not in fact taken or received meat. The difficulties of ascertaining what in fact had been consumed by the Indian soldier were often insuperable even to those who had worked with Indian forces all their lives, and it was unsafe to attempt to draw conclusions from the official ration scales.

Contrary to views often expressed, I could seldom get much evidence of troops arriving in the country who were already "on the scurvy line" as the phrase went. In two representative series of cases, about 250 altogether, the men who had reported sick in consequence of scurvy within three months of reaching Mesopotamia were under 8 per cent.

Another noteworthy point of the epidemic was the great difficulty in effecting any rapid cure of scurvy once definite symptoms had declared themselves. It was this that made the outbreak so serious from a military point of view, not only in cases which had got to the stage of hæmorrhages, but those at earlier stages when there was little but anæmia and spongy gums to notice. Some men of course were glad to have a disease which meant that they would be sent out of a detestable country. But others who were keen to be cured and to stay in Mesopotamia, native officers, old soldiers, kohars at ambulances and so forth, who willingly took special curative antiscorbutic diet and conformed to treatment, often failed in the end.

Pellagra has, if one may trust recent American work on its dependence on protein starvation, now again to be classified with the deficiency diseases. The only one outbreak which I heard of in the Eastern campaigns was not among the troops, but among Armenian refugees in Egypt in 1917.

OTHER EPIDEMICS, AND CONCLUSIONS.

If I have dealt mainly with epidemic sickness special to the East, it must not be supposed that the forces on the different fronts escaped the epidemic diseases common at home. It is true that scarlet fever, measles, and whooping-cough, if they occurred, certainly did not come prominently to notice, but diphtheria on the other hand was often rather extensively prevalent amongst British troops both in Egypt and Mesopotamia. Mumps was a source of much invaliding and sometimes spread with an intensity one seldom sees at home outside schools and institutions. Ghurka units in Mesopotamia were very severely hit by mumps, as also some Australian units in Egypt and native labour corps. Small-pox, as might be expected from the system of vaccination, seldom got any footing when introduced; its occasional prevalence gave reason for some careful consideration of methods for preventing the loss of strength of vaccine lymph when kept and sent about in hot climates. Cerebrospinal fever, though it did not spread widely, caused some anxiety in Egypt in 1915, especially among New Zealand and Australian troops, and in Basra at the end of 1916.

If I were to stretch somewhat the ordinary use of the term epidemic, I might add a great deal as to the waves of sickness which were occasioned by the effects of heat in the summers of Mesopotamia, or by cold as felt in the Balkans in the winter of 1915-16. I have, however, already given too long a list for this address. It will not, I trust, be thought that by referring to the epidemics which have occurred I have given them any disproportionate prominence and omitted to call your attention sufficiently to those which might have occurred but have been prevented.

The debt of the Army to preventive medical work has already been eloquently dealt with by lecturers and speakers at general meetings of this Society, and it is not necessary for me to describe the mass of devoted individual labour which has been put into the prevention of these diseases by those on the spot aided by those at home. These have been men in the field, in the hospital, in the laboratory, and at headquarters; not only the specialists, but men who have the knowledge and administrative capacity to balance up and apply the known facts as to the natural history of diseases to secure their most effective prevention in the circumstances of the moment: an applied epidemiology which is really one of the highest and most difficult branches of the science with which our Section is concerned.

All sciences have been called in to play their part in the War: those who have pursued epidemiology in the Eastern campaigns, like their colleagues in France and at home, have cause to rejoice in the evidence that their own science is not among the least of those which have aided the fighting forces and our country's cause.